plished in the year 1883, and that many of the lacunæ in our knowledge are being steadily filled in. The Molluscoidea seem to have had more than ordinary attention paid to them, and the record of this group by Prof. E. von Martin appears to be extremely well done. As usual, Messrs. W. H. Kirby and R. McLachlan record the enormous section of Insecta, the lion's share falling to the former, the latter confining his attention to the Neuroptera and Orthoptera. In his treatment of the general subject (Insecta) the recorder frequently quotes memoirs relating to the structure, &c., of the groups recorded by Mr. McLachlan, and it is not without interest to note that, while some of these are the subjects of a double record, others are not. One interesting fact, showing the importance which a "Zoological Record," when complete, is to the working naturalist, is alluded to by Mr. McLachlan in his remarks introducing us to H. de Saussure ("Mémoires pour servir à l'Histoire naturelle du Mxeque des Antilles, et des États-Unis. Orthoptères de l'Amérique moyenne: Famille des Blattides." Genève, 1864):—"This very important memoir is noticed at the request of the author. It escaped notice in the early volumes of this 'Record' (which commenced with the year 1864), and also in the German *Bericht*. It would also appear to have escaped the notice of workers on Blattidæ generally, for none of the new terms employed therein for generic, &c., division are included in Scudder's just-published laborious 'Universal Index' which extends down to 1879." Scudder's New Index is, however, far from being a full record of generic names in any one group.

The new names proposed for genera or sub-genera, as recorded in this volume, amount, the editor informs us, to 1079, as against 1015 of last volume, and this without including any of the Arachnidæ. Of these, no less than 115 require re-naming, having been already in use. This number affords no clue to the amount of new species described, which is considerably larger, thus indicating for the present no lack of work for the systematic zoologist.

The British Association for the Advancement of Science still continues its grant of 100*l*., and the Government Grant Committee of the Royal Society renewed its vote of 150*l*., while the Zoological Record Association itself keeps up both the number of its members and subscribers.

A Treatise on Practical Chemistry and Qualitative Inorganic Analysis. By Frank Clowes, D.Sc. Lond. Pp. xv., 376. Fourth Edition. (London: J. and A. Churchill, 1885.)

THIS well-known manual has reached a fourth edition. It very thoroughly fulfils the aim which is set forth in the preface, viz. to place trustworthy and practical methods of qualitative analysis in the hands of the student. If the chemical student must still devote a large amount of his time to qualitative testing, then he certainly could not do better than follow the directions of this book. But the very excellence of the tables and methods of the book before us makes us more than ever doubt the wisdom of attempting to teach the science of chemistry by a course of "test-tubing." The art can be learnt by rules and formulæ, but the science comes not by such as these.

This book only includes what "directly bears on the ordinary requirements of the laboratory student"; its directions are those of a man who knows what he is writing about, and who has learnt what he teaches by good honest work in the laboratory. It contains many of those results of laboratory experience which are usually preserved in the private note-books of the teacher, and which may almost be regarded as trade secrets. The only fault we have to find is that the book tends too much in the direction of recipes. Were a student to work conscientiously through the book he would certainly be an accomplished analyst, but we are afraid he might have ceased to be a chemist. However excellent rules and tables may be in their own way, it is possible to have too much of them. In fact, the better they are the less one wants to be bound by them. The "tables of differences" given in the book are excellent; in the hands of a good teacher they might be made the basis of a really scientific training. But the ordinary student will not trouble to develope methods from the facts set before him in these tables; he will pass on to the systematic examination of simple salts, and be caught in the fatal whirlpool of "experiment," "observation," "inference." M. M. P. M.

Original Researches in Mineralogy and Chemistry. By J. Lawrence Smith. Edited by J. B. Marvin. (Louisville, 1884.)

IN a recent number (vol. xxxi. p. 220) we gave a statement of the life and work of the late Prof. J. Lawrence Smith condensed from a memoir prepared at the request of the National Academy of Sciences, Washington, by Prof. B. Silliman, who was so soon to follow his friend to his long rest. The papers containing the original investigations of Prof. L. Smith have now been collected together and reprinted as a memorial volume intended for presentation to his friends. Three memoirs prepared by Mr. Marvin, Mr. Michel, and Prof. Silliman respectively, form an appropriate introduction, and give one a good glimpse into his life and character. The work is clearly printed on good paper, and will be highly appreciated by his numerous friends, to each of whom a copy has been presented by his widow.

Lehrbuch der Mineralogie. Von Dr. Gustav Tschermak. Zweite, verbesserte Auflage. (Wien: Alfred Hölder, 1885.)

WE are glad to find that a second edition of this work is already called for, although the latter part of the first edition appeared so lately as 1884. In our notice of the first part of that edition (vol. xxiv. p. 355) we directed attention to the excellent character of the work, and gave a brief statement of its contents; we now need only remind our readers that the author is a thorough master of his subject, who has done a large amount of original and valuable work, and further, has had a long teaching experience as Professor of Mineralogy in the University of Vienna. The work is but slightly changed in the present edition; the length is increased by a few pages through the incorporation of the results of investigations made since the first part left the press in 1881; the contents are well up to date. If some University Professor would provide us with an equivalent work written in our own tongue the study of mineralogy in this country would begin to revive.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Mr. Lowne on the Morphology of Insects' Eyes

I DESIRE to give an unqualified denial to the imputation made by Mr. Lowne in his letter to NATURE of April 9 (p. 528), that my opinion with regard to his paper on the structure of the eye in Arthropods was formed under the influence of my colleague, Prof. Lankester, or that any consultation upon the paper took place between us. References of papers for the Royal Society are strictly confidential, and I did not know the name of the second referee until after I had come to a conclusion upon the subject—a conclusion which was only arrived at as the result of a long and patient investigation of Mr. Lowne's preparations,

and the nature of which may be inferred from the fact that I advised the author to withdraw his paper and submit the subject to a renewed investigation, with the aid of improved methods.

With regard to the question, which Mr. Lowne raises, as to my competency to form any opinion at all, on the ground that I had not myself devoted any special attention to the literature of the subject, I may remark that the points which had practically to be decided were (1) whether Mr. Lowne's statements were in themselves probable, and (2) whether they were corroborated by his preparations. Had I not felt myself qualified to form an opinion on these points I should not have accepted the reference.

E. A. SCHÄFER

The Late Prof. Clifford's Papers

IN the "Mathematical Papers" (pp. 628–37) I was able to print the syllabuses of a series of ten lectures delivered by Prof Clifford to a class of ladies at South Kensington in the spring and summer of 1869. Whilst turning over a collection of miscellaneous papers, in a box, Mrs. Clifford and I had the good fortune to light upon a manuscript quite ready for printing, and this ("Mathematical Papers," p. 628) subsequently formed part of the volume on "Seeing and Thinking;" but we could not find any trace of any more manuscript of the above-mentioned series of lectures. Just before the recent Easter holidays Prof. Karl Pearson returned to me a few pages of manuscript bearing on the International Scientific Series volume which I had lent him, and with them he sent me a large note book which had been in the late Prof. Rowe's hands. On opening this book I at once saw that it contained very full notes of other lectures of the course. In fact, Lecture II. ("On Plane Surfaces and Straight Lines") is quite ready for press, as is also, I think, Lecture III. ("On the Rotation of Plane Figures"); Lecture IV. ("Of Similar Figures") is a fragment, and still more fragmentary is Lecture V. ("The First Principles of Calculation"). Of Lecture VI. ("The Theorem of Pythagoras") there are two loose sheets of figures: on one sheet is "the Bride's Chair," and the figures on this and the other sheet show that my information was correct, and that the remarks on pp. 633, 637 are *ad rem.* As Lecture IX. ("On the Shadows of a Circle") is very fully illustrated in the recent volume edited by Prof. Pearson, we see that we are in possession of a fairly complete presentment of Prof. Clifford's views on the subjects of the course of lectures.

Messrs. Macmillan have stated their willingness to publish the MS. of the second part of "The Elements of Dynamics," and I hope to be able, after a re-examination of it, to put the work into their hands for printing. When this book is got out, and the above lectures published in some shape yet to be determined, the mathematical world will be in possession of all that we can now look for from the hands of this great master.

University College School, April 25 R. TUCKER

Sir Wm. Thomson and Maxwell's Electro-magnetic Theory of Light

SHORTLY after writing my former letter I saw a copy of the verbatim report of Sir Wm. Thomson's lectures in Baltimore, and would have written to you to that effect and to apologise to Mr. Forbes for having doubted the accuracy of what I thought was his report, only that I met him in London about that time, and he then desired me not to do so. Sir William Thomson has now himself stated that the passage is correctly quoted, and I can only regret that he has expressed himself in the way he did.

I certainly think that anybody reading the passage would imagine that the velocity of propagation of electro-magnetic disturbances upon Maxwell's electro-magnetic theory of light, which he showed to be the same as the velocity of propagation of light, and to be a true velocity of wave-propagation—any one, I say, would suppose that this was the same thing as that Sir Wm. Thomson calculated in the year 1854.

Sir Wm. Thomson certainly says, "That is a very different case," but the rest of this sentence is rather ambiguous as to what the "it" after "putting" refers to, and I am afraid that many people will imagine that, in Sir William Thomson's opinion, Maxwell has made some unjustifiable assumption. I believe, however, that all he thinks is that Maxwell has not made a satisfactorily definite thing of the so-called electro-magnetic theory of light.

In Sir Wm. Thomson's article in Nichol's "Cyclopædia" he puts the matter very clearly indeed. He says:—"The law of this phenomenon [transmission of electric signals] is identical with that which Fourier . . . found as the law of propagation of summer heat and winter cold to different parts of the earth," *i.e.* it obeys the laws of a diffusion and not of a wave-propagation; and again:—"Now it is obvious from these results [experimental results] that the supposed velocity of transmission of electric signals is not a definite constant like that of light:" and afterwards he says that, when an initial current is started, the potential rises simultaneously at all points, and that the apparent velocity would depend on the delicacy of our instruments. All these obviously distinguish between the propagation of a variable current in a conductor and a true wave-propagation.

He has also clearly pointed out a direction in which to look for a true wave-propagation. It will make his position clearer, and also Maxwell's, to use his analogy between water in an elastic tube and a conductor of electricity. I will suppose the water contained in a tube bored out of a very large lump of india-rubber. He enumerates three electric qualities concerned, and their hydrodynamic analogues:—(1) "Charge" or electrical accumulation in a conductor subjected in any way to the process of electrification. (2) "Electro-magnetic induction" or electromotive force excited in a conductor by variations of electric current. (3) Resistance to conduction through a solid. The hydrodynamic analogues are:—(1) Accumulation of a greater or less quantity of water in any part of the canal or tube. (2) Inertia of the water. (3) Viscosity or fluid friction. He explains that a true wave-propagation arises from the compressibility of the water, combined with its inertia, and that if the tube be elastic, like india-rubber, there would also arise a wave-propagation. "Accordingly," he says, "a definite velocity of propagation of electric impulses, depending on the inertia and the capacity for charge, is to be looked for, as has been done in a first article, published by Kirchhoff, on the subject."

Now, in all this discussion Sir Wm. Thomson omits to mention the only thing that is at all analogous to Maxwell's propagation of wave disturbances in non-conductors, and it arises from his considering the water as contained in a tube like ordinary india-rubber tubes, instead of in a tube bored in an indefinitely large lump of india-rubber. If we consider this case it is evident that one of the conditions to be considered is the propagation of waves in this lump of india-rubber. In Sir Wm. Thomson's tube there would of course be a velocity of wave propagation in the india-rubber, but that is a very different matter from the propagation of disturbances away from the neighbourhood of the tube by which energy would be carried away from it. To do this Sir Wm. Thomson should have included the propagation of sound in the air or whatever he supposed surrounding the outside of his tube. Without including this, he was not including anything a bit analogous to Maxwell's electromagnetic theory of light. In Sir Wm. Thomson's tube the whole state of affairs at any time could be expressed in terms of variables that represented bodies near the tube, while in the other case it would be absolutely necessary to introduce variables representing every part of the india-rubber which I have supposed of indefinite extent. This is just the difference between Sir Wm. Thomson's and Maxwell's views. According to Maxwell's view there is a great deal more going on outside the conductor than inside it, and it is evident that the inertia of the water is a very bad analogue to electromagnetic induction, for this latter depends essentially upon the form of the circuit, and not only upon its section and length. Maxwell has shown that light may be a wave-propagation of what are on his theory *analogous*, though probably utterly *unlike* the distorsional waves propagated in the india-rubber, and has shown that a medium which would only transmit disturbances analogous to these would explain electric and magnetic phenomena. It is to be remembered that Maxwell's theory gets rid of all action at a distance, and that the only *experimentum crucis* between theories of action at a distance and of action through a medium is that in this latter case the energy may be propagated in time through the medium, while in the former it cannot.

I cannot conclude without protesting strongly against Sir Wm. Thomson's speaking of the ether as *like* a jelly. It is in some respects *analogous* to one, but we certainly know a great deal too little about it to say that it is *like* one. May be Maxwell's conceptions as to its structure are not very definite, but neither are any body's as to the actual structure of a jelly, and there is no real difficulty in supposing a medium whose condition is